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Published in:
Social Epistemology: 5 Questions

2015

[Link to publication](#)

Citation for published version (APA):
Olsson, E. J. (2015). Answers to 5 Questions in Social Epistemology. In V. F. Hendricks, & D. Pritchard (Eds.), *Social Epistemology: 5 Questions* (pp. 145-159). Automatic Press.

Total number of authors:
1

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Answers to Five Questions in Social Epistemology

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1. Why were you initially drawn to socio-epistemic phenomena?

My original interest was in epistemology in the traditional individualistic sense. This led me to study the concept of epistemic coherence, the property which beliefs have when they in some sense “hang well together”, a topic which I wrote my dissertation on (Olsson, 1997). In the dissertation I studied coherence from the point of view of theories of belief revision, which was a hot topic at the time. Belief revision was then largely synonymous with the AGM theory of Alchourrón, Gärdenfors and Makinson, and its variants. In the AGM theory, belief revision is studied from a logical point of view, and the resources available are basically sentential logic plus set theory. A belief state is represented as a set of sentences, or set of possible worlds, together with some way of representing the notion that different beliefs can be more or less firmly held (more or less “entrenched”) in a given belief system. Consequently, the resources available for capturing the concept of coherence were also limited. In particular, I sensed a lack of a notion of “evidential independence”: the idea that two or more pieces of evidence originated from independent sources. I thought, as others have done before me, that coherence has epistemic force only if the items that cohere have some degree of evidential independence.

After my PhD, I turned to probability theory as a framework for studying coherence. It turned out that this was a good move because I discovered, much to my delight, that the missing notion of independence could actually be captured in probabilistic terms, namely as a form of conditional independence. My first influence came from a paper by Peter Klein and Ted Warfield in *Analysis* where they presented a probabilistic argument to the effect that coherence cannot be truth conducive (Klein and Warfield, 1994). Their argument was that a set of observations can often be expanded with an explanation. Such an expanded set is more coherent, from an explanatory point of view, than the original smaller set. But, unless the explanation follows from the observations, which would be an unusual case, the new set will be less likely to be true as a whole than the original. Hence, coherence is not truth conducive. However, I thought there was something fishy with this argument. The inquiry that followed led me to study C. I. Lewis’s work on coherence in his book from 1946. One interesting fact about Lewis – and this is where social epistemology enters the picture – is that he takes a case of several reporters or witnesses reporting the same story as the paradigm case of coherence. Thus, he departs from the traditional view that coherence is prototypically a property of an individual inquirer’s beliefs. However, Lewis grants of course that we can also apply the concept of coherence to individual beliefs or memories (his main application in the end). Those beliefs or memories are then seen as analogous to separate witness testimonies in court. This idea of Lewis’s opened up the possibility of studying witness scenarios on an abstract probabilistic level and applying the findings to the traditional problems of epistemology, mostly skepticism, in which the concept of coherence has been traditionally put to use. This I did in my later work on coherence, which culminated in my book *Against Coherence* (Olsson, 2005). I return to the arguments of this book below.

My more recent work on social epistemology was inspired mainly by two sources. One was

the Hegselmann-Krause simulation model of collective inquiry, especially their 2006 paper, and the other Alvin I. Goldman's theory of what he calls veritistic social epistemology, as laid out with characteristic scholarly excellence in his *Knowledge in a Social World* from 1999. My own contribution can be seen as an attempt to combine these two approaches into one or, more precisely, to devise a simulation model of inquiry which would, in the spirit of Goldman, use Bayesian updating and allow for the epistemic value of social practices to be objectively assessed. Let me explain.

In their 2006 paper, Hegselmann and Krause, inspired by Lehrer and Wagner (1981), proposed a non-Bayesian model for determining the chances for the truth to be found and broadly accepted in situations in which only some inquirers are reliable. The communication process was taken to consist in a mutual exchange of opinions between all individuals, reliable or not. H&K assume that there is a true opinion, T, in the space of possible opinions that may be capable of "attracting" individuals to various degrees. An interesting result that came out of their work was that even if only some of the individuals are reliable, the group will gradually approach a consensus position which is close to the truth, although it may not be the exact truth. However, the model does not take into consideration the group members' assessment of the reliability of the other members, although that assessment is bound to be important in the overall persuasive effect of a deliberative contribution (e.g. an argument or the statement of an opinion). The model, moreover, is based on a linear method for updating opinions that seems to lack independent standing in the philosophical literature. Having read Hegselmann and Krause's paper, I thought there was room for improvements in these regards, as I explain in Olsson (2008). In particular, a more convincing model would allow for the inclusion of trust as a factor the updating process and using standard Bayesian updating would be an improvement upon the updating rule used by these authors. As I recall it, this was the first motivation for us – Staffan Angere who was a PhD student at the time and I – to start working on a Bayesian simulation model of social networks.

The second motivation came from Goldman's work in social epistemology. In his 1999 book, Goldman outlines a theory for how to evaluate social practices with respect to their "veritistic value", i.e., their tendency to promote the acquisition of true beliefs (and impede the acquisition of false beliefs) in society. Goldman's main proposal is that degrees of belief (DB) have veritistic value relative to a question Q, so that any DB in the true answer to Q has the same amount of V-value as the strength of the DB. In Goldman's terminology, $V\text{-value of } DB_x(\text{true}) = X$. Suppose, for example, that Mary is interested in the question whether it will rain tomorrow. If the strength of Mary's belief that it will rain tomorrow is .8, and it will in fact rain tomorrow, then the V-value of Mary's state of belief vis-à-vis the rain issue is .8. This idea is then extended to cover social practices, e.g. relying on experts, trusting one another etc. Goldman's main proposal in this regard is that the veritistic value of a practice can be computed as the average over the veritistic values of all applications of the practice. In the same work, however, Goldman raises a number of serious worries for his account. Two of them concern the possibility of determining the veritistic value of a practice in a concrete case because (1) we often don't know what beliefs are actually true, and (2) even if we did, the task of determining the veritistic value would be computationally extremely difficult. Thus the second goal we set ourselves when developing our Bayesian simulator was that it should be able to compute veritistic values of interesting social practices automatically, thus solving Goldman's computational problem, at least in principle.

2. What are your main contributions to this field of study?

My main early contribution to socio-epistemic issues was my inquiry into coherence in a probabilistic setting, taking Lewis's witness scenario as a paradigm case. This work

culminated in my 2005 book. As I explained, the book, and the papers upon which it was based, combined individualistic and social epistemology in one philosophical inquiry. In the book, I argued, for instance, that while Klein and Warfield's argument against the coherence theory was flawed, a version of their conclusion could actually be shown to hold. It could be shown that coherence, properly understood, is not truth conducive, in the following sense: there is no measure of coherence such that a higher degree of coherence is positively correlated with a higher posterior probability that the statements in the set are true (i.e. given that they have been reported by witnesses or, analogously, form the contents of a given subject's beliefs). I have continued to write on coherence and witness agreement, although less intensely than I used to. For a recent example of a paper on the role of witness coherence and reliability in law see Schubert and Olsson (2012).

My recent work has focused on developing our Bayesian simulator and applying it to various issues in social epistemology. As for the first project, my role has often been to suggest various changes and additions to the simulator. These proposals have then been implemented by Angere (who is not only an outstanding philosopher and mathematician but also an excellent computer programmer, a rare and valuable combination of talents). Angere came up with the basic underlying Bayesian model, Laputa, and has made numerous changes and improvements to the simulator in the course of its history. He is mostly to be credited for the simulator bearing the same name, whereas my main contribution – apart from those already noted – has been to find interesting applications. I should mention that the way in which Laputa treats trust – as a second order probability or, more precisely, a credence in objective reliability – is reminiscent of how I conceived of witness reliability in my later work on coherence.

I see Laputa as my main contribution to social epistemology so far, and since I am still excited about it I have decided to include some details about the simulation framework here (based on the exposition in Olsson, 2011). The reader is advised to consult Vallinder and Olsson (2012) regarding the underlying probabilistic model. The model was originally laid out in Angere (to appear), but at the time of writing this paper has not yet appeared in print.

A basic notion in Laputa is that of a *social network* in which people can communicate with each other. Social networks are represented as graphs in which the nodes represent inquirers and the links represent communication channels. The links are directed, allowing for one-way communication. Figure 1 is an example.

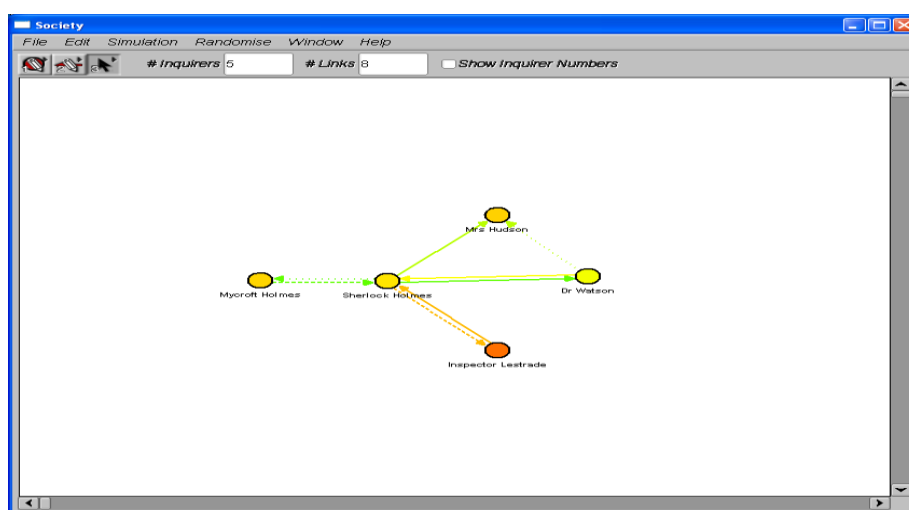


Figure 1: The (characteristically exclusive) social network of Sherlock Holmes as represented

in Laputa.

Following Goldman, it is assumed that all inquirers focus on answering one and the same question: whether p or not- p . For example, p can be the proposition “The economic crises will soon be over”. The *initial degree of belief* is an inquirer’s credence (subjective probability) in p from the start. *Inquiry accuracy* is the reliability of the inquirer’s own inquiries. The *inquiry chance* is the probability that the inquirer will conduct an inquiry. The *inquiry trust* is the inquirer’s degree of “self-trust”, i.e., her degree of trust in her own inquiries. Likewise, there are a number of parameters for each link. The *communication chance* is the probability that the sender will send a message given that her degree of belief exceeds the *threshold of assertion*. As for the latter, if the threshold is set at .90, this means that the sender needs to believe p (not- p) to a degree .90 in order for her to “assert” p (not- p) in the network. The *listen trust* is the recipients trust in the sender.

Running Laputa can mean to construct a network such as that in Figure 1, assign initial values to the inquirer and link parameters, and then click on a “run” button. This triggers Laputa to run through a series of steps, each step representing a chance for an inquirer to conduct an inquiry, to communicate (send, listen) to the other inquirers to which she is connected, or to do both. After each step, Laputa will update the whole network according to the information received by the inquirers. This is done in accordance with standard Bayesian techniques. Thus, a new degree of belief is computed for each inquirer based on the old degree of belief and the new information received through inquiry and/or listening to other inquirers. Laputa also updates the inquiry trust and listen trust parameters in accordance with Bayesian principles. After such a process of network updating, Laputa computer the veristic value of the network evolution as the difference between the average final and initial degrees of belief. A positive value means that there was an increase in veristic value: the process of inquiry and communication brought people somewhat closer to the truth on the average.

However, the veristic value of a particular *network evolution* is perhaps not that interesting. What we would like to do is to assess the veristic value of a *practice*. The first thing to note is that what we have learned about Laputa so far allows us to study the V-value of a particular *application* of a practice. Consider for instance the practice of trusting other people. Before we run the network we can adjust the listen trust parameter for all the links so that this condition is satisfied. Now we run the network as previously described, preferably until the network stabilizes and relatively fixed degrees of belief have been obtained. What we get as a result is the V-value of the practice of trusting other people as applied to the particular network at hand and its initial state (e.g. the Sherlock Holmes network of Figure 1).

Laputa now solves the problem of computing the veristic value of a practice as an average over the veristic values of all its (considered) applications. It does so by allowing its user to specify various features or “desiderata” of networks at an abstract level. The program can then randomly generate a large number of networks, of different sizes, having those features, letting them evolve, collecting the corresponding V-values and, finally, outputting the average V-value of all the network evolutions it has examined. This allows Laputa to compute the V-value of a large number of interesting practices. For instance, Laputa can be told, at the abstract level, to study 10,000 randomly generated networks in which inquirers trust each other to a certain degree. The resulting V-value is a measure of the V-value of the practice of trusting other people to that degree, independently of any particular network. All this is done in Laputa’s “batch window”. Further details about how this works can be found in Olsson (2011).

Apart from allowing the veristic value of practices to be determined, the development of Laputa had two conceptual bonuses that should be highlighted. First, Laputa can differentiate

between the short run and long run veritistic performance of a practice. Suppose for example that we want to know how beneficial truth telling is in the long run. This problem could be studied by setting the number of steps per trial to, say, 100. If we are more interested in short term uses, we could instead set the number of steps to a smaller number, say, 5 or 10.

Secondly, Laputa can help us to get clearer on what a social practice is. From the point of view of Laputa, a social practice can be identified with a *network constraint*. Any such constraint which can be imposed in Laputa's batch window can be studied from the point of view of veritistic value in the manner just described. This includes constraints that would perhaps not normally be described as social practices, e.g., "being reliable in one's inquiries to degree .75". Nevertheless, identifying a social practice with a network constraint may still be a fruitful *explication* of the concept of a social practice, in the sense of Carnap (1950). Laputa can be downloaded for free from <http://sangere.users.sourceforge.net/>.

The model was proposed first and foremost as a *normative* model of group deliberation and communication. Nevertheless, it has been argued that group communication in Laputa exhibits a number of characteristics of real life group communication, including polarization, i.e. the process whereby "members of a deliberating group predictably move toward a more extreme point in the direction indicated by the members' predeliberation tendencies" (Sunstein, 2002). For this point see Olsson (2013) and, for a study of the role of overconfidence, Vallinder and Olsson (forthcoming). Laputa has been applied to a number of different philosophical issues. It is applied to Goldman's computational problem in Olsson (2011). It is applied to the problem of norms of assertion in epistemology in Olsson and Vallinder (2013), inspired by the account in Douven (2006). And it is applied, finally, to the Argument from Disagreement in ethical theory in Vallinder and Olsson (2013).

3. What is the proper role of these studies in relation to other disciplines?

This is obviously a huge subject. I will confine myself to just one aspect of it: the relation between socio-epistemic studies, or social epistemology, and traditional epistemology. The background is Alvin Goldman's view that social epistemology should be assimilated to epistemology in the traditional sense because social epistemology is, as he puts it, "real epistemology" (Goldman, 2010). I am inclined to question this view. Obviously, this is not in any way a dismissal of social epistemology as such; I would be the last person to make such a move. It is only a claim that the two – social and traditional individualistic epistemology – are different disciplines and should, at least for the time being, be viewed as such.

In his 2010 paper, Goldman surveys a number of issues in social epistemology that are introduced roughly in order of increasing degree of "sociality". For starters, testimony and peer disagreement are certainly social phenomena, although they are so in a comparatively innocent sense. Here the focus is still on the individual. It is just that her individual inquiry is placed in a wider social context. The next step up the social ladder is to study institutions with respect to how good they serve the epistemic purposes of individuals that aspire to attain truth and avoid falsehood. This step is significant because it represents a shift of attention from the individual person to the social institution and its design. We can proceed still further. Several researchers have maintained that it makes good sense to ascribe knowledge, belief, acceptance and the like not only to individuals but also to groups. Goldman is open to this suggestion, pointing out that it would raise interesting issues of how individual judgments are aggregated to form collective ones.

Now Goldman thinks that we can ascend up this social ladder and still do epistemology in the traditional sense, at least so long as there is continuity with what he takes to be the *core assumptions* of traditional epistemology, such as the objectivity of truth and the central role of normativity and rationality. Let us refer to this requirement as that of *individual-to-social*

continuity. A further criterion that he introduces is that the social phenomena we are considering, like the Internet or judicial tribunals, should have causal influence on individual inquirer's doxastic attitudes. I will call this condition that of *social-to-individual causality*.

Let me now turn to my first critical remark. Goldman, as I just mentioned, favors a characterization of epistemology in terms of certain "core assumptions". I agree that there is, on this characterization, much continuity between Goldman style social epistemology and traditional epistemology. After all, this brand of social epistemology is a normative enterprise; and it is predicated on the assumption of truth being an objective, largely mind-independent, affair, and so on for several other core ideas. What I want to suggest, however, is that the picture changes if we, as indeed I believe many practitioners do, think of traditional epistemology, not or at least not only in terms a set of core assumption but also in terms of a set of *core issues*. If we do, the case for including much of social epistemology in traditional epistemology seems less compelling. Let me try to explain why.

What are the core issues in traditional epistemology? Well, the problem of skepticism would certainly be one, as would the problem of accounting for the nature of knowledge. And then we have or course the traditional rationalist-empiricist debate concerning the identification of legitimate *sources* of knowledge. We shouldn't forget the value problem: why is knowledge distinctively valuable? Then there is the question regarding the *limits* of knowledge. Are there truths that cannot be known? This list of core issues could of course be extended.

Now it seems far from obvious that social epistemology has much to offer by way of answering these and other core issues of traditional epistemology. The problem of the nature of knowledge, for instance, does not seem to involve in any crucial way a social dimension. Neither does the problem of the limits of knowledge. At least this is what I anticipate that most mainstream researchers would say about these cases. (I will return to social accounts of the core issues at the end of this remark.) So, Goldman's argument for his main thesis depends in large measure on a characterization of traditional epistemology that I believe many mainstream epistemologists would find seriously incomplete. I take this claim of mine to be in accord with a broadly Kuhnian view on what constitutes a research paradigm, where the core issues – the questions that are considered to be most urgent – play a pivotal role for that purpose.

My second point will be that Goldman's argument seems to prove too much. My reasons for this claim draws on an analogy with the social sciences. The science of psychology is defined in Encyclopedia Britannica as a "scientific discipline that studies mental processes in humans and other animals". There we also learn that "the issues studied by psychologists cover a wide spectrum, comprising learning, cognition, intelligence, motivation, emotion, perception, personality, mental disorders, and the study of the extent to which individual differences are inherited or are shaped environmentally". Sociology, on the other hand, is claimed to be "a social science that studies human societies, their interactions, and the processes that preserve and change them". Furthermore, "it does this by examining the dynamics of constituent parts of societies such as institutions, communities, populations, and gender, racial, or age groups". Finally, "sociology also studies social status or stratification, social movements, and social change, as well as societal disorders in the form of crime, deviance, and revolution".

Now clearly, there is, on this description, much continuity between psychology and sociology so that, to quote from the same source, "[t]he broad nature of sociology causes it to overlap with other social sciences such as ... psychology". For instance, both aim at accounting for various aspects of human behavior, including various forms of "disorder". Moreover, the groups studied by sociology causally influence the behavior of the individuals studied by psychology. Indeed, it is according to this authoritative source "sociology's task to discover how organizations affect the behavior of persons".

On an abstract level, then, we have both individual-to-social continuity and social-to-individual causality. If Goldman were right in thinking that these very features are those that justify assimilating the social to the individual, sociology should be considered real psychology. But, as we know, it isn't.

The third point, or rather a question, is: what does it take for the social to be assimilated to the individual? The answer to this question, I believe, lies not or at least not only in individual-to-social continuity and social-to-individual causality. For the social to be assimilated to the individual, there has to be, I submit, a general feeling that the core issues in the individual domain cannot be satisfactorily dealt with, not even in approximation, without bringing in the corresponding social dimension. In a word, there has to be a general sense of *anomaly* in the individual domain. The assimilation of social psychology to psychology bears witness to the fact that some issues in psychology are indeed such that they cannot be even be approximately accounted for from a purely individual perspective.

A parallel case in epistemology is its relatively recent assimilation of a theory of testimony. The inclusion of testimony in mainstream epistemology is due to the fact that, thanks mainly to Coady (1992) and his followers, it has become increasingly clear that the traditional account of the sources of knowledge in terms of perception, memory, intuition, and so on, is utterly incomplete. Unless we take testimony to be a full-fledged source of knowledge, we must conclude that we have much less knowledge than we thought we had. In this way, the traditional problem of accounting for the sources of knowledge simply could not be solved without bringing in, as a last resort, some social machinery. It follows that for social epistemology *as a whole* to become a legitimate extension of traditional epistemology, it must be shown to have crucial bearing on a substantial number of core issues in the individual domain. *Pace* Goldman, it doesn't suffice that it complies with most of the core assumptions.

Here, then, is how the current situation looks to me: Traditional epistemology and social epistemology are different areas of philosophical inquiry defined partly by their different sets of core issues. However, they share an interest in the epistemology of testimony which may, for reasons already given, be legitimately viewed as real traditional epistemology. Maybe the same is true also of the epistemology of disagreement without which, it could be maintained, any theory of justification would be radically incomplete, although I am less sure in that case. At any rate, once we have accepted the epistemology of testimony as real epistemology, it can also be invoked to shed light on another traditional issue: the value of knowledge. Clearly, knowledge is valuable in part because it gives the person who has it the right to transmit it to others via the speech act of sincere assertion or testimony. Having such a right is a good thing. Surely, any theory of the value of knowledge would, in the end, need to accommodate this social observation. So testimony is special in the sense that it is intimately tied to two fundamental problems of mainstream epistemology. Small wonder that it could be assimilated without much protest! Yet apart from these central problems concerning the sources and value of knowledge, respectively, there is to my mind no clear further candidate for socialization among the traditional core issues. So, as things stand, I am afraid that I do not fully share Goldman's optimism regarding the prospects of a more far-reaching inclusion of social epistemology within epistemology as traditionally conceived. Social epistemology is valuable and it has this value largely independently of the core issues of traditional epistemology.

4. What have been the most significant advances in the field?

I should say at the beginning that, coming to the study of socio-epistemic phenomena and social epistemology rather late, I lack a comprehensive overview of the now rather extensive field. The following remarks will have to be rather impressionistic, I am afraid. At any rate, I tend to think of the "veritistic" contributions to social epistemology as the most important

ones. By that I mean the kind of social epistemology in which a robust notion of truth plays a major role. I am thinking, for instance, of Nicolas de Condorcet's early jury theorem. Condorcet considered a case of assembly members voting independently on a proposition p , e.g. the proposition that the accused is guilty. He assumed, further, that the voters are at least somewhat reliable and that they are reliable to the same degree, r (e.g. $r = 0.6$). He then showed that the probability that the majority is correct exceeds r and, moreover, that this probability converges to 1 as the number of voters goes to infinity. In other words, the view of the majority is more likely to be correct than the view of an individual voter, and the greater the number of voters, the more reliable is the voice of the majority. There have been important recent extensions of and elaborations on these results by Christian List, Robert E. Goodin and others (e.g. List and Goodin 2001, Goodin 2003). I take the study of coherence, as I have described it above, to be mainly a further extension of Condorcet's pioneering work. It extends Condorcet's work e.g. by allowing witnesses to be in less than full agreement. The basic model studied involves independent but somewhat reliable witnesses giving coherent reports, and the question is whether a report set which is more coherent (or exhibiting greater agreement) is thereby more likely to be true *ceteris paribus*. Apart from work already mentioned, Luc Bovens and Stephan Hartmann's 2003 book *Bayesian Epistemology* is a major contribution to this field.

A second branch of veritistic social epistemology considers social practices more generally (and with less mathematical rigor), including free speech and legal procedure. The first major work in this area is generally taken to be John Stuart Mill's famous treatise *On Liberty*, which provides a systematic and still influential defense of free speech from the point of view of the value of truth. Goldman's aforementioned *Knowledge in a Social World* belongs, as I see it, to this second branch of veritistic social epistemology. Chapter 7 of Goldman's book contains a thorough treatment of the subject of free speech.

The "new wave" in this area, from my perspective, is to use computer simulation techniques to study the, often very complex, relationship between various social practices and the dissemination and acquisition of true belief. As I mentioned, Hegselmann and Krause has done seminal work in this field, drawing on Lehrer and Wagner (1981). It should be added that Lehrer and Wagner did not actually study belief convergence from a veritistic point of view. Truth plays no role in their model. Hegselmann and Krause should be credited for bringing in truth into the picture. Rainer Hegselmann has continued to do important work in this field, as have, for instance, Igor Douven (2010) and also Kevin Zollmann (2007), who was inspired by Bala and Goyal (1998). For instance, Douven has applied a Hegselmann-Krause style model in an enlightening way to the debate over disagreement in mainstream epistemology. Douven and Kelp (2011) is a useful overview of recent work on computer-aided social epistemology.

5. What are the most important open problems in the field and what are the prospects for progress?

There are a surprising number of societal issues that are important but which do not seem to fall into any particular established discipline. On closer examination, these issues may turn out to be of a social-epistemological kind. One example, among many, is how to evaluate and possibly improve judicial procedures. We obviously want judicial procedures to end in true judgments, so that the accused is sentenced if and only if he or she is actually guilty. What seems to have happened in many countries is that a judicial system has evolved over the years in a trial and error fashion with little or no systematic oversight of the judiciary as a whole. One example is the problem of jury size. How many jurors should be involved in a trial? This is a strikingly simple question but if one look at different countries, one tends to get very

different answers. In Sweden, there are four jurors participating in a district court trial. In the US and the UK, there are usually 12 jurors. In Scotland, the number is 15, and so on. Curiously, few legal scholars seem to know the reason why a particular number was adopted. The likely explanation is “because this is the way it has always been”.

So, is there such a unique number from an epistemological point of view? If social epistemology could have something to say on this issue, this could be hugely beneficial because nobody else seems to be in a position to address it. This said, there is useful work in organizational and experimental psychology that may be relevant. To add to the (local) urgency of this matter: in Sweden there seems to be a developing consensus, judging by the debate in the public domain, that the judicial system, or a substantial part of it, is in need of revision. For example, in a recent (unscientific) poll most professional Swedish judges expressed their dissatisfaction with the layman jury system (“nämndemanssystemet”). Yet, there is no consensus regarding who should be revising it, or what information should be brought to bear on that decision. I am not saying, or course, that social epistemologists should be taking over the revision of the judiciary. However, I do think that some of the things we are doing can be relevant in such a process.

One of the more recent Laputa studies addresses the problem of assessing the optimal number of jurors in a trial (Angere and Olsson, unpublished). Our preliminary results support the Scottish 15 juror system. While adding another juror always adds epistemic value, there is a diminishing marginal return, and beyond 15 jurors there is little surplus epistemic value to be had by adding one more. If one takes into account the administrative cost of adding another juror or the fact (well-known to organizational psychologists) that the size of the group tends to be negatively correlated with the efforts of its members, results may go in the direction of smaller juries.

I mention this in order to make a general point: This unpublished study somehow received unexpected attention in Swedish media (e.g. Lind, 2012). I do not take this to be an indication of the importance of the results, which are rudimentary at best. Rather I take it to be an indication that there is so little evidence to draw on in this area that any new results that have a touch of objectivity will be welcomed and discussed. Very likely, other simulation frameworks could provide valuable input here as well, and I encourage their inventors and users to work on the problem. At any rate, here is an area where society is in great need of assistance and where social epistemologists seem to be sitting on, or being in the process of developing, some significant tools.

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